

Fate and transport of sediment and mercury in the upper Yuba River watershed for current and future climatic conditions

Lorraine E Flint

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0207: Fate and transport of sediment and mercury in the upper Yuba River watershed for current and future climatic conditions

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The PI's describe a study that will use a hydrologic model to better understand sediment and Hg transport in the Yuba river watershed. The proposal directly responds to a priority area highlighted by the CBDA in the call for proposals. The PI's propose to calibrate a sediment transport model, develop a relationship of sediment size and Hg distribution, model potential land use and climate change scenarios and transfer this application to other river and reservoir systems. There is a strong research team assembled and the potential to gain sufficient knowledge about Hg movement through the watershed is essential to predicting long term fate and bioavailability of this toxic trace metal. Unfortunately, this proposal appears to have been assembled in haste and the result is one that is difficult to follow and lacking of major details. The sections are extremely uneven and reviewers lacked a sense that this was truly a cohesive project. Information appeared to be missing (description of "hillside erosion device", details on provenance studies using magnetic information, etc.). It was also difficult to determine which aspects of this study have been completed during the previous study. It is obvious that the previous study lacked high flow events necessary for extending the modeling efforts, but the proposal does not make a strong enough case for having the optimal

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methods for determining effects of these events (much is riding on the use of OBS, but no preliminary data give it justification for Hg). Reviewers were in agreement that this proposal was rated "good" at best and have provided sufficient information to consider for a resubmission.

Additional Comments:

The PI's describe a study that will use a hydrologic model to better understand sediment and Hg transport in the Yuba river watershed. The proposal directly responds to a priority area highlighted by the CBDA in the call for proposals. The PI's propose to calibrate a sediment transport model, develop a relationship of sediment size and Hg distribution, model potential land use and climate change scenarios and transfer this application to other river and reservoir systems. There is a strong research team assembled and the potential to gain sufficient knowledge about Hg movement through the watershed is essential to predicting long term fate and bioavailability of this toxic trace metal. Unfortunately, this proposal appears to have been assembled in haste and the result is one that is difficult to follow and lacking of major details. The sections are extremely uneven and reviewers lacked a sense that this was truly a cohesive project. Information appeared to be missing (description of "hillside erosion device", details on provenance studies using magnetic information, etc.). It was also difficult to determine which aspects of this study have been completed during the previous study. It is obvious that the previous study lacked high flow events necessary for extending the modeling efforts, but the proposal does not make a strong enough case for having the optimal methods for determining effects of these events (much is riding on the use of OBS, but no preliminary data give it justification for Hg). Reviewers were in agreement that this proposal was rated "good" at best and have provided sufficient information to consider for a resubmission.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Fate and Transport of Sediment and Mercury in the Upper Yuba River Watershed for Current and Future Climatic Conditions

The panelists felt that the proposal was not well-written. External reviewers did agree on this. The external reviews were considered thorough by the panel. The panelists felt that the researchers are strong hydrologic modelers. However, several aspects of the methodology were not described thoroughly. One reviewer expressed concern that the entire application of the model for mercury transport was dependent on grain-size relationships, yet there was no data in the proposal to suggest there is in fact a true relationship. The panel agreed that this was a basic flaw in the proposed work.

The proposal was considered uneven by the panel. Some parts, in particular the provenance aspects were considered better than other parts. The problem of identifying mercury sources was considered important by the panel. The model, however, did not incorporate source material relationships into transport.

Reviewers felt that the links between the data collection and the modeling were not clear, thus decreasing the probability for a successful project.

Rating: adequate

Technical Review #1

proposal title: Fate and transport of sediment and mercury in the upper Yuba River watershed for current and future climatic conditions

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The proposed project is a continuation of a previously funded CALFED project evaluating sediment and mercury transport in the Yuba River watershed, and several of the stated objectives have already been accomplished to some degree. After reading the full proposal, it becomes clear that a major motivation for continued funding was a lack of large streamflow events during the previous monitoring period. The three years (2001-2003) during which data were collected in the previous project all featured very small peak annual flows (Figure 2 of the proposal), and thus the project team was not able to develop confidence in either their sediment rating curves, their measured mercury vs suspended sediment relationships, or their conceptual sediment transport model. The need for additional high-flow data to substantiate previous findings is not made clear in the goals and objectives section. Additionally, several fundamental and important proposed activities are not mentioned in the goals section. For example, project task 4, quantifying hillslope erosion processes, and project task 5, provenance studies, are not mentioned in the goals. There are good ideas in the proposal, and the issues seem important and relevant to management of mountain streams in gold mining areas, but the proposal suffers organization problems that make it difficult to decipher exactly what is proposed and why.</p>
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Technical Review #1

Rating	fair
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Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The proposal is built on a large knowledge base developed by the investigators during their previously funded CALFED project. From the earlier project, the investigators have written several USGS publications and at least one published journal article. Furthermore, the investigators have a solid grasp of the relevant scientific literature, and they have developed a good conceptual model of sediment sources and mechanics in the watershed. The proposal does not address the biological concerns regarding mercury transport in the watershed, but overall the justification for the proposal is sound.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The various proposed tasks vary considerably in the quality of the descriptions of the approaches. For example, Task 2, suspended sediment sampling, and Task 5, provenance studies, are well-described, documented, and justified. At the other end of the scale, it is nearly impossible to discern what techniques will be applied to Task 4, quantifying hillslope erosion processes. The proposal says, "At each site... a hillslope erosion collection device will be
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	installed." This reviewer has considerable experience with sediment budgets and has no idea what a "hillslope erosion collection device" might be. The proposal provides no citation for this method. The proposal ignores the issue of quantifying mass wasting, which is likely a significant source of sediment in this watershed (and a component of the proposal's conceptual sediment budget). Similarly, the proposal provides no rationale for the selection of tributaries to be monitored in Task 3, tributary data collection to evaluate spatial distribution of suspended sediment. After studying this watershed for three years, the investigators must have some knowledge of the basin that has motivated their selection of four tributaries for analysis, but the logic behind this choice is not provided. For a proposal seeking 1.4 Million dollars, this reviewer would expect more detail on methods.
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Given the investigators experience with the basin and with many of the proposed techniques, it is highly likely that the investigators will be able to improve the sediment transport models and learn more about sediment deposition in Lake Englebright. It is not clear from the proposal, however, that the investigators will be able to quantify sediment sources accurately. Specifically, Tasks 2, 5, 7, and 8 seem feasible and well within the investigators' capabilities, while Tasks 3, 4, and 6 still seem rather nebulous.
Rating	good

Technical Review #1

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	See comments in the approach section.
Rating	poor

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The past and present mining activities in the Sierra Nevada mountains create sediment issues not present in most salmonid-bearing streams of the Pacific Northwest. Better information on mercury sources, storage, and transport in these systems should be useful to water resource managers in California.
Rating	very good

Additional Comments

Comments	From section to section, the quality of this proposal is highly variable. There are aspects of this proposal that are very good while other areas seem to have been written in a hurry and are thus difficult to review. If CALFED is interested in extending this work, this reviewer suggests funding only those aspects of the proposal that have been well developed and justified. The other areas should not be funded until the investigators have better developed the goals and approaches.
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Technical Review #1

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The investigators' publication record from the previous funding period indicates they are well-qualified to conduct most of the proposed work (with the caveats above).
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget is quite large. Costs for Tasks 2 and 7 seem high. Costs for Tasks 4 and 6 are difficult to assess because of the poor description of the approaches for these tasks. The most expensive single task is Task 5, provenance studies, at \$404,000, which is probably reasonable given the scale of the proposed effort. The costs for Task 8 seem reasonable.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	Much of the proposed work is justified due to the importance of mercury issues in the Yuba River and similar rivers and due to the need for supplemental high flow data to substantiate previous findings. The proposed provenance studies and the reservoir sedimentation studies appear well considered and scientifically important. However, other aspects of the proposal are so weak that it is difficult to give the overall proposal a high score.
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Technical Review #1

Rating	good
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#0207: Fate and transport of sediment and mercury in the upper Yuba River wat...

Technical Review #2

proposal title: Fate and transport of sediment and mercury in the upper Yuba River watershed for current and future climatic conditions

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	Goals are clearly stated and internally consistent. It would have been preferable to see specific hypotheses that the researchers were interested in testing, rather than simply indicating that they will 'hypothesize' about certain topics (see objectives 1-3.) No objectives specifically address provenance studies (task 5), yet this component of the study is significant in terms of the budget (almost 1/3). The general idea of developing a well-calibrated, physically-based watershed model for sediment and hydrology is valuable and it is important to characterize as best possible the mountainous watersheds that contribute to bay-delta streamflow, sediment, and water quality.
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The background information indicates that this study builds solidly upon previous research and will address gaps and problems with the current understanding of sediment-hydrology dynamics in the watershed. The
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Technical Review #2

	conceptual model (fig. 4) provides the general context for their study, albeit without the temporal component they wish to address with climatic modeling. Given their objectives, a research-level project is justified.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	At the broad level, the research approach described is clear and reasonable. However, it would be beneficial to either have more methodological information (sometimes difficult due to page constraints) or at the least citations indicating the methods that will be used. For example, under Task 4, what type of "hillslope erosion collection device" will be use and how will they determine the "relative contributions from mass wasting and surface erosion"? Results of this study will likely add to the current watershed process understanding and the provenance study (task 5) and optical backscatter testing (task 2) will test relatively new methodologies with respect to this study. A better understanding of watershed sediment dynamics, specifically for bay-delta source watersheds should be useful for managers and policy makers in this region.
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Technical Review #2

Comments	As mentioned above, the proposal would benefit from more methodological details, but the overall feasibility seems high given information provided. The provenance study (task 5) and optical backscatter testing (task 2) are experimental, while the remaining tasks are more traditional and highly likely to produce successful results. Experimental topics are valuable since they advance our scientific understanding. The scale of the project is consistent with the objectives and appears to be within the scientific expertise of the authors.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Not applicable.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The authors clearly indicate their expected products/deliverables, which emphasize reports. As long as the reports are professionally published (likely through the USGS although not specified) this should produce valuable and accessible research contributions. Authors indicate that this work will be a strong contribution to the Upper Yuba River Studies Program, a component of the CALFED Ecosystem Restoration Program.
Rating	good

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Technical Review #2

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The authors are successfully publishing their work from a previous Upper Yuba River investigation in good journals, indicating a solid track record. The team clearly has the expertise to carry out this study. They do not address if they have the available infrastructure and other aspects of support necessary to accomplish the project, but the budget indicates only reasonable and relatively inexpensive materials/equipment requests.
Rating	very good

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget seems reasonable for the work proposed. However, the central and critical role of the experimental provenance study should have been explained in more detail in the proposal to justify it accounting for almost 1/3 of the budget. It currently reads as a valuable and interesting experimental contribution to the study, but not a critical component to completing overall objectives.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

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Technical Review #2

Comments	This study will clearly contribute to a better understanding of sediment dynamics in the Upper Yuba River Basin and is a valuable overall contribution to understanding source basin contribution to the bay-delta ecosystem. However, the proposal could be improved with more information on important aspects such as hypotheses and methods.
Rating	good

Technical Review #3

proposal title: Fate and transport of sediment and mercury in the upper Yuba River watershed for current and future climatic conditions

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goals and objectives are laid out on pages 7 and 8 of the document. A watershed model is to be developed that will couple flow, sediment transport and mercury flux in the Yuba River basin in response to a noted watershed hydrologic and chemical data deficiencies in CALFED studies. Specifically, the proposal indicates that the project will:</p> <p>1) calibrate a sediment transport model to understand the distribution of sediments throughout the watershed, 2) develop relationships between mercury and grain size, 3) test climate and land use change scenarios using the model, 4) evaluate reservoir depositional models and make the results transferrable to other Sierra Nevada rivers and reservoirs.</p> <p>These objectives are clearly stated at the top of page 8. For some reason, they are repeated at the bottom of page 8/top of page 9 verbatim, and are paraphrased as project purposes on page 8.</p> <p>There are no hypotheses associated with this project per se, although specific objective 1 suggests that after the sediment transport model is calibrated, the authors will "hypothesize about how to spatially distribute sediment loads throughout the watershed, which will increase the uniqueness of the model results...". It is unclear to me what is meant by this</p>
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Technical Review #3

	<p>statement.</p> <p>The authors are clear and deliberate in the coupling of this proposal to specific CALFED 2004 priority areas that focus on the development of quantitative approaches linking climate and land-use change to watershed hydrology, sediment and mercury transport processes, and further monitoring to lower uncertainty about the spatial distribution of Hg loads throughout the watershed, sediment storage and transport, and delineation of suitable habitats with regard to contaminants such as sediment and mercury.</p> <p>As such, the project goals as stated are timely and address these CALFED priority areas. There is a sense however, that the specific project objectives are presented to satisfy the priority topic areas, as opposed to a formulating a tightly coherent set of related scientific questions.</p>
Rating	fair

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	<p>The study is justified, in that it is required to provide process-oriented parameterization for an HSPF model developed as part of the the Upper Yuba River Studies Program (UYRSP). This HSPF model appears to model flow quite effectively, as discussed in the proposal, but requires specific erosion information, and information about sediment transport dynamics and chemistry in order to utilize the model more effectively for sediment transport modeling. This information is to be derived from the proposed work.</p> <p>The conceptual model for the study is that provided in a USGS science report and presented in Figure 4, and</p>
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Technical Review #3

	outlines the sediment reservoirs and fluxes in the Yuba River watershed. The proposed work intends to examine the important processes mechanistically. Although it is not immediately apparent, an equally important aspect of the proposed work is to use magnetic parameters to interpret provenance of sediment deposits to reconstruct depositional and reworking history of hydraulically mined mercury rich sediments. The conceptual model illustrates the underlying basis for the proposed work in that it illustrates all of the various compartments and processes are involved in the watershed-scale sediment transport regime. The proposal as a whole fails to come across as a project that must be implemented as a whole. Both the more empirical studies and the sediment provenance work could be approached under separate work efforts.
Rating	fair

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The approaches to addressing the knowledge gaps identified above are presented in the Work Plan section.</p> <p>ACTIVITY 2: Task 2. Improvement of sediment rating curves using optical backscatter sensors (OBS). The refinement of sediment rating curves must be a high priority to the proposed research, and attempts to utilize automated sensor technologies to improve sediment flux estimates is an important effort. This approach would appear meritorious, as Figure 10 indicates a significant divergence between estimated SSC from a 'calibrated' optical sensor and that from a rating curve. Given that there are confounding factors</p>
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that affect the OBS measurements that are identified in the proposal, there are significant challenges presented. Since example data are given in Figure 10, it is unclear to me why the word "may" appears in the first sentence on Page 14 which states, "Thus, rating curves may misrepresent sediment transport...". It is fairly apparent to me that the sentence should read "Rating curves misrepresented sediment transport ...". The lack of discharge dependency on some of the sedigraphs presented is highly curious and worthy of investigation. Clearly a more well-developed model of sediment transport may help to address these discrepancies.

Task 3: Tributary sediment data collected to verify the conceptual model. I am not sure how one verifies a conceptual model of sediment transport by sampling two events, since the conceptual model of upland tributaries as given includes generic stores and fluxes except for inputs such as dam failures. I think that this is a case of overwriting the proposal; it would be more clear to simply state that no data exist for episodic sediment transport in the upland tributaries, and this data is required to parameterize the sediment transport model. This will definitely add to the base knowledge.

ACTIVITY 3: Evaluation and Quantification of Sediment Sources

Task 4: Quantifying hillslope erosion processes. Hillslope erosion is to be measured at five hillslope sites in the basin using "hillslope erosion collectors". It is unclear what is meant by this, why five sites, how many events, what sort of device is to be used, etc. Why has hillslope erosion been prioritized over bank erosion, for example? How important is mass movement? I would think that sheet flow or gully erosion might be modeled using sediment cohesion parameters, grain size, slope, roughness and moisture content? No citations from the erosion

Technical Review #3

literature are provided for methodological support. This is clearly an important component to the model, but this section does not elaborate sufficiently to evaluate.

Task 5: Provenance studies. The proposal states that the study of the sediment transport, deposition and reworking history using geochemical (and magnetism studies, by my assessment) are largely experimental, but could yield very valuable information about the sources of sediments (mined vs non-mined) to the Yuba river, and potentially lead to the application of sediment source mapping information to other watersheds. Although not fully articulated, the proposed task may yield valuable information about sediment deposition history.

ACTIVITY 4: Distribution and Quantification of total mercury

TASK 6 - Grossly unsatisfactory level of detail is provided in this section. The section refers to a figure of suspended sediment grain size and mercury concentration that demonstrates that further effort on this is futile (i.e. no relationship) at 3 of 4 stations shown! Considering the emphasis on mercury in the title and summary of this proposal, this section is written as an afterthought. How important is the sediment Hg flux versus the dissolved flux in this system? Obviously very important, but this is not stated anywhere. "We will hypothesize about how to spatially distribute the suspended sediment and mercury loads using GIS". Do the authors really mean that an outcome of this effort will be the ability to sit around a table and hypothesize about this?

Activity 5: Calibration and application of model

TASK 7: Calibration and application of watershed flow and transport model

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	<p>This section is the core of the proposal. Calibration of the model will entirely depend on reliable empirical measures of sediment flux which will hopefully be provided by the OBS and other field measurements. As such, this activity is highly dependent upon the success and applicability of the field components. This is conceptually fine, but there is no fall back position presented if the empirical data prove insufficient.</p> <p>The application of the model to mercury transport is dependent upon the mercury-grain size relationships, which are for the most part non-existent according to the data presented. Therefore this approach is flawed. Imposing future climate scenarios on the flow and sediment transport model using climate simulation output is perfectly feasible, but it assumes that the flow and sediment transport model are working well, and it also assumes that the projected climate changes do not force the model out of its calibration range where non-linear or unexpected results may be expected.</p> <p>A proportionally huge work effort is proposed to examine sediment deposition in the reservoir and subsequent transport. Without going into detail, this section was clearly written by another author, as it actually provides good detail and methodology. As such, this section represents a reasonable sub-study that could be supported which would provide useful information about seasonal changes in sediment deposition.</p>
Rating	poor

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	
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	Only the OBS sediment calibration described in Task 2 and the reservoir study in the final section described under Task 7 is technically feasible, in that it is fully documented. The HSPF modelling that has been undertaken is promising, but the 'filling in the gaps' exercise proposed here with respect to assessing within basin sediment sources and mercury transport is not documented sufficiently to truly assess, as describe.d above in the Approach comments.
Rating	poor

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The measurement program (e.g. hillslope erosion, Hg-grain size tests), as described, are inadequate as discussed in the Approach comments above. The data derived may be insufficient to aid in model calibration.
Rating	poor

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	In principle, the sediment calibrated HSPF model would represent a valuable contribution to the understanding of the dynamics of sediment (and Hg) transport in this system. I am not confident that such a final product is within reach as proposed, but if accomplished, represents a good contribution.
Rating	good

Additional Comments

Comments	The proposal is sloppy. Figures are out of order, and in some cases, mis-numbered (e.g. Figure 8 as described in the text is labelled Figure X, Figure 9 is labelled Figure 8, Figure 10 is labelled Figure 8, etc.). The Background section jumps awkwardly between discussion of sediment transport processes, the magnetic sediment provenance discussions, the previous development work of the HSPF model, then back to the reservoir, making it difficult to follow the development of the problem statement of the proposal.
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	Although L. Flint is the only PI listed at the top of the proposal, I presume that all other personnel listed are co-PIs. L. Flint has considerable expertise in hydrology and the measurement and parameterization of hydrologic properties at various scales. Given this background, the lack of articulation of methodologies in the proposal is surprising. Publication record is strong; funding record is either non-existent or not included in the CV for some reason. J. Curtis appears to be fulfilling a technical role on the project, with expertise in GIS, sediment processes. Alpers is a very important figure in geology, mine wastes and mercury. Being a rigorous and respected scientist, I have difficulty believing that he had a substantive contribution to this proposal, given the weakness of the mercury Task description. Snyder is responsible for Task 4.3, (which does not exist?) but who has expertise in sedimentary processes. A. Flint has published extensively and has appropriate expertise for the tasks proposed. Bouse will be performing analyses of isotopes and has an impressive publication record. There is no CV module for Dettinger, Eberl, Not PIs?
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Technical Review #3

Rating	very good
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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Salary and benefits make up the majority of the budget. The rates are assumed to be correct, There is no indication of how the number of hours of commitment were determined for each task or each participant. Under some non-labor sections (e.g. Task 6), equipment is vaguely described ("Containers, \$300.00"). The budget is appropriate, if the work effort of these participants toward the outcome of this project in its entirety is deemed appropriate.
Rating	good

Overall

Provide a brief explanation of your summary rating.

Comments	This is a fairly weak proposal with uncertain outcomes. It seems like an awkward collection of sub-proposals that were assembled for this one. Some of the mis-numbering, redundancy, and errors (e.g. the missing TASK 4.3) support this. As indicated in the feasibility section, there may be components that could be considered for pilot funding. The coupling of the sediment sources to the HSPF model seems largely unfeasible, and the mercury aspect of the proposal is for the most part, non-existent. I cannot recommend the proposal as written.
Rating	poor

